Magnetic Fields and Cancer

Editor's Note: The following letter is a response to an article by Savitz and Kaune, EHP 101:76–80.

In 1979 Wertheimer and Leeper developed their W-L wire code, based on power line construction, size, and distance from a residence, as a surrogate for residential magnetic field exposure (*1*). This W-L wire code was applied to data from a 1988 Denver study, and an association was reported between "high" W-L wire code and childhood cancer (*2*). The authors of "Childhood Cancer in Relation to a Modified Residential Wire Code" (*3*) use the 1988 Denver data and report a stronger association between their newly defined "high" wire code and childhood cancer. However, they report that this new wire code is not more strongly correlated with measured magnetic fields. (The authors state that both the W-L wire code and the new wire code can explain only 20% of the magnetic field variability.) A stronger association with childhood cancer without a correspondingly stronger correlation with magnetic fields suggests that factors other than magnetic fields may be involved in the reported wire code association with childhood cancer.

The authors suggest that their new wire code is less susceptible to classification error than was present with the W-L wire code and that, had all of the 1988 Denver study homes been classified correctly, the W-L wire code would have produced a stronger association with childhood cancer. However, a "Back-to-Denver" (BTD) study, based on a subset of the 1988 Denver homes, found the W-L wire code classification error rate to be "quite small" (*4*). This finding appears to weaken the authors' argument that classification error resulted in an appreciable reduction in the association between W-L high wire code and childhood cancer in the 1988 Denver study. The authors of the BTD study conclude that factors other than magnetic fields may be responsible for the reported wire code association with childhood cancer (*4*).

Explanations exist that suggest the reported stronger childhood cancer association with the new wire code may be false. For example, the selection criteria used in the original study (*2*) caused the case and control populations to be noncomparable with respect to residential mobility. Specifically, controls were required to be residentially stable from case diagnosis to selection as a control but cases were not. Thus, an artificial association between residential mobility and cancer was created by the subject selection procedure. This artificial association was shown to be a possible explanation for at least part of the reported association between W-L high wire code and childhood cancer (*5*). An even stronger bias from differential residential mobility might exist with the new wire code. This possibility could be tested by restricting the analysis to only those cases and controls that were residentially stable *from diagnosis to matched control selection*. Children that lived in homes in whlch magnetic field measurements were taken were residentially stable from diagnosis to control selection (*2*) and could provide the basis for such a comparison.

Another possible explanation for a false association between wire codes and cancer is that individual cases may not have been matched with a control from the same neighborhood. In the 1988 Denver study children with the same telephone exchange as the case were eligible for selection as controls (*2*). The area corresponding to a particular telephone exchange likely does not correspond to a single neighborhood. For example, in Columbus, Ohio, the telephone exchange for the oldest neighborhood (developed in the 1800s) includes modern residential areas that were annexed to the city after 1970. Neighborhood differences between the cases and controls have the potential to significantly influence wire code associations because wire codes have been found to be strongly associated with the age of a neighborhood. For instance, 50% of the homes in one of the oldest neighborhoods (inner city) in Columbus were classified as W-L high wire code, while less than 25% of the homes in

Table 1. Wire code classifications and residential age

	Wertheimer-Leeper[a]			Kaune-Savitz new wire code[b]			
Line construction	Distance (feet)	Classification	Residential age[c]	Distance (feet)	Classification	Residential age[c]	Result
All transmission and thick wire	<130	High	Mix	<65	High	Older	
three phase distribution primary	≥130	Low	Newer	65–150	Medium	Mix	Fewer newer homes in high category
Thin wire three phase distribution	<65	High	Older	<65	High	Older	
primary	≥65	Low	Mix	65–150	Medium	Mix	Unclear
First span open wire secondary	<50	High	Older	<85	Medium	Older	
	≥50	Low	Older	≥85	Low	Older	Fewer older homes in low category
First span spun wire secondary	<50	High	Newer	All	Low	Newer	
	≥50	Low	Newer				Fewer newer homes in high category and more newer homes in low category
Other open wire secondary	All	Low	Older	<85	Medium	Older	
				≥85	Low	Older	Fewer older homes in low category
Other spun wire secondary	All	Low	Newer	All	Low	Newer	No difference
No overhead wires within 150 feet of residence	All	Low	Newer	All	Low	Newer	No difference

[a]Savitz et al. (*2*).
[b]Savitz and Kaune (*3*).
[c]Jones et al. (*5,6*). "Older" and "newer" are relative terms that are indicative of the age of the neighborhood in which a particular power line construction is more commonly found. "Older" generally means residential neighborhoods that were developed more than 50 years ago, while "newer generaly refers to areas less than 50 years of age. "Mix" imples a mixture of older and newer neighborhoods. Older residences are more likely to be classified as "high" wire code and less likely to be classified as "low" wire code than newer residences. This likelihood differential appears to be greater for the Kaune-Savitz wire code than for the W-L wire code.

two neighborhoods developed within the last 50 years were classified as W-L high wire code (*6*). Based on these Columbus data, if cases from the inner city are matched with controls from newer neighborhoods, odds ratios exceeding a value of 3.0 could be produced, falsely associating high wire code with childhood cancer.

Evidence exists that the cases in the 1988 Denver study may be from older neighborhoods relative to the controls. For example, of the homes classified as W-L high wire code based on secondary powerline construction, 63% of the cases and only 33% of the controls had the older "open wire" construction (*7*). This finding is suggestive of a failure to match cases and controls by neighborhood, which could have resulted in the creation of a false association between W-L high wire code and childhood cancer.

As compared to the W-L wire code, the authors' new wire code appears to place an even greater proportion of older powerline constructions in the high wire code category and a greater proportion of newer constructions in the low category (Table 1). Based on the authors' Table 3, the most important result may be the movement of spun wire secondaries (present standard construction which was introduced in Columbus in the 1950s) within 50 feet of a residence from the W-L "high" category to the new "low" category (23% of the W-L "high wire code" controls, while only 9% of the W-L "high wire code" cases were so reclassified). If cases are from older neighborhoods relative to the controls, application of this new wire code would likely result in a stronger but false association between the new high wire code and childhood cancer. Adjustment for age of neighborhood should remove this possible bias.

In conclusion, the fact that the new wire code is only weakly correlated with magnetic field measurements (in the same manner as the original W-L wire code) suggests that the newly reported stronger association with childhood cancer is likely due to factors other than magnetic fields. Differential residential mobility and differential residential age are two possible explanations and are suggestive that the reported association may be false.

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References

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Response: Potential Bias in Denver Childhood Cancer Study

Jones makes several points in his letter to that we would like to address.

The observation that the modified wire code is similar to the Wertheimer-Leeper code in its relation to measured magnetic fields, yet more strongly related to cancer, is interpreted as evidence that both wire codes reflect some exposure other than magnetic fields as the basis for their relation to cancer risk, but the modified wire code does so more effectively. If the measured magnetic field were the true gold standard, this reasoning would be valid, but the real interest is in long-term, historical magnetic field exposure to occupants of the residence, which is unfortunately not available. If the modified wire code is superior, then its relation to the gold standard exposure would presumably be enhanced, but not necessarily the relation to spot magnetic-field measurements, another imperfect surrogate of exposure.

The "Back to Denver" study (*1*) is cited to verify that the Wertheimer-Leeper wire code can be reliably assessed. However, that study did not directly address the question of which aspects of the coding system are contributory and which are superfluous, thereby adding only random error relative to the ideal exposure measure. The greater simplicity of the new system is one of its expected contributions, allowing less skilled persons to collect valid data, but we also believe that the approach may eliminate some distinctions that are not of importance in estimating exposure. The reduction in misclassification would not be solely due to fewer actual recording errors but in more accurately and parsimoniously reflecting the field-determining characteristics of the power lines. A number of alternative explanations for the wire code–cancer association are considered by Dovan et al. (*1*). Unfortunately, the data reported cannot be used to prove that magnetic fields or some factor other than magnetic fields account for the observed associations.

In a recent article (*2*), the hypothesis was put forth that differential residential mobility accounts for much of the association we observed originally between wire codes and childhood cancer (*3*). Jones et al. argue that 1) controls in our study in Denver were restricted to be residentially stable from the date of the matched case's diagnosis to the time of selection (a period of 0–9 years, depending on the corresponding case's date of diagnosis); 2) data collected in Columbus, Ohio, demonstrate an association between residential stability and wire configuration code. Occupants of homes with wire codes indicative of elevated magnetic fields are less stable; 3) application of the differential mobility by wire code in the Denver study produces an odds ratio due to selection bias of around 1.5.

Given that cases were ascertained over an 8-year period (1976–1983), which preceded data collection (1984–1985), control selection posed a challenge. If all residents of the study area at the time of selection were considered eligible, we would have included many children who had moved to the area subsequent to the corresponding case's age of diagnosis. We chose instead to restrict controls to those who were present when the case was diagnosed and remained in the area until the time of selection. We recognized that this omitted controls who would have been eligible at the time of diagnosis but who had subsequently moved away, and acknowledge that this constitutes a potentially important source of selection bias in the study (*3*). Data gathered by Jones et al. (*2*) in a different city and time period from our study provide a firmer empirical basis for such a concern, but the question of generalizability from Columbus to Denver cannot be made with certainty. Organization of cities with respect to land use, socioeconomic status, and patterns of migration are complex and quite likely to be distinctive, especially in different regions of the country.

A comprehensive analysis of our data to address the role, if any, of selection bias related to mobility is underway, but several points raised by Jones are in error. We restricted controls to be stable from the time of diagnosis to the time of selection, whereas cases were included whether stable or mobile during that period. As a result of this requirement, there was a small imbalance in the prediagnosis period (birth to diagnosis): 82 of 224 interviewed cases remained stable (37%), whereas 81 of 198